

case. Exp. 5, p. 10, does not clearly show that the heat due to chemical union is independent of the heat caused by the lamp; while statements 6 and 7, if taken together, produce in the pupil's mind a confusion between cause and effect. In pp. 23, 25, the allusions to acid and alkali, both new names to the student, might easily be accompanied by a marginal reference to p. 65, where the terms are explained. In Exp. 17, p. 27, it is not made evident in words that the hydrogen has passed from the one bottle into the other. Exp. 22, p. 34, and Exp. 40, p. 68, would gain in value if the action of the text, as well as its result, were described, such explanation of the first experiment being actually given later on at p. 87. In Exp. 29, pp. 44, 45, the diction of the first two paragraphs is confused and confusing, and it is doubtful if any lecturer would be able to conduct Exp. 31, p. 48, so as to retain the heated filings on the magnet. In Exp. 35, p. 56, the numbers on the drawing do not tally with those of the description. The explanation of the Davy lamp, p. 57, to which further reference is made in Physics, p. 86, is, to say the least of it, incomplete: and that of the safety matches on p. 72 is quite unintelligible. The definition of an Element, p. 58, and the phrase "difficultly fusible" on p. 99, suggest purely verbal criticisms.

In the Physic Primer, p. 2, force and motion should hardly be called "qualities" of dead matter. In Exp. 13, p. 22, the "simple arrangement" for moving the horizontal piston might be indicated. In p. 23 the large and small piston are not lettered in the description. In Exp. 17, p. 26, a shrewd pupil would inquire why the upward pressure should not, from all that appears in the text, expel water from the higher aperture as strongly as the downward pressure expels it from the lower aperture. In pp. 40, 41, some allusion to the Aneroid, if not to the Hypsometer, might fairly be expected, and in p. 46 it is not easy to see why the Syphon is described if its principle is not to be explained. In p. 65 there is an allusion to "the mercury in the bulb of glass," which is, in fact, there mentioned for the first time, and is it not true that in Exp. 52, p. 105, a principle of converse action is laid down on the evidence of a specific and almost a solitary instance?

It is possible that to criticise these points as blemishes suggests stupidity to the critic; if so, his stupidity is probably typical, and the authors would be the first to wish that their explanations should be self-sufficing, even to the obtuse. In any case we tender them our hearty thanks for work which marks a stage in the advance of scientific education. Its lingering progress hitherto has been owing to the want, not of zealous champions, but of united action. The labours of its advocates are now beginning to converge. The leaders of science and the leaders of education are drawing close together—on the one side eager to impart, on the other ready to receive, advice and guidance. By the publication of these books the most serious of the obstacles which have kept them separate is removed.

W. TUCKWELL

OUR BOOK SHELF

Astronomy and Geology compared. By Lord Ormthwaite. (London: J. Murray, 1872.)

THIS little volume is the product of a thoughtful and observant mind. Its main object is to contrast the

certainly of the conclusions of astronomy, the exactitude with which eclipses can be foretold, and with which other astronomical phenomena recur, and the vagueness which hangs round many geological theories, as, for instance, those connected with the age of the various strata. A large portion of the volume is directed against the theories of Mr. Darwin in natural science, and Mr. Buckle in morals, theories which the author considers, in consequence of the vagueness of geological conclusions, to rest upon insufficient data. With the general mode in which the argument is conducted, we have little fault to find. Occasionally, however, Lord Ormthwaite's zeal on behalf of orthodox theology betrays him into injustice, as when he says:—"There is one feature common to the writings of Mr. Darwin and Mr. Buckle which is to be regretted—they both of them seem to ignore, if they do not altogether deny, the existence of a First Cause. Secondary causes are always with them the only springs of motion." With this we may contrast the following sentence from the "Origin of Species":—"To my mind it accords better with what we know of the *laws impressed on matter by the Creator*, that the production and extinction of the past and present inhabitants of the world should have been due to secondary causes, like those determining the birth and death of the individual." Lord Ormthwaite pleads bodily infirmities as an excuse for any inaccuracies or mistakes in the book, and we very willingly allow the plea.

The Use and Origin of the Arrangements of Leaves in Plants. By Chauncey Wright. (American Academy of Science and Arts.)

THIS is an elaborate and ingenious attempt to apply the principles of Natural Selection, or the Survival of the Fittest, to the observed phenomena of Phyllotaxy, or the arrangement of leaves on the stems in plants. Stating in the outset very clearly the distinction between this theory, according to which every organ, and every arrangement of organs, must be of some practical (though possibly undiscovered) utility to the plant, and that of "types," which requires no such hypothesis, Dr. Wright proceeds to investigate how the origin of the phenomena under investigation can be accounted for on the former theory. It must be assumed in the outset that the two principal modes of the arrangement of foliar organs, of which all others are modifications, the spiral and the verticillate, are modifications of a single original type. Investigating the actual arrangements on mathematical principles, he finds that the various angular distances of leaves on the stem are resolvable into the general form of

the continued fraction
$$\frac{1}{a + \frac{1}{1 + \frac{1}{1 + \frac{1}{1 + \dots}}}}$$

in which a may have the values 1, 2, 3, or 4. The actual fractions thus resulting are when

$$\begin{aligned} a = 1 & \dots \frac{1}{2}, \frac{2}{3}, \frac{3}{5}, \frac{5}{8}, \frac{8}{13}, \text{ \&c.} \\ a = 2 & \dots \frac{1}{3}, \frac{2}{5}, \frac{3}{8}, \frac{5}{13}, \text{ \&c.} \\ a = 3 & \dots \frac{1}{4}, \frac{2}{7}, \frac{3}{11}, \frac{5}{18}, \text{ \&c.} \\ a = 4 & \dots \frac{1}{5}, \frac{2}{9}, \frac{3}{14}, \frac{5}{23}, \text{ \&c.} \end{aligned}$$

each fraction being obtained by adding together the numerators and denominators in the two preceding fractions. Practically it is found, however, that certain only of these fractions occur in nature, while of those that are found some are much more frequent than others. The approximate ultimate value (k) of this continued fraction, when $a = 1$, is 0.6180, k possessing the property that any power is equal to the difference between the two next lowest powers, or $k^n = k^{n-2} - k^{n-1}$. On this peculiar arithmetical property of k depends the geometrical one of the spiral arrangement which it represents, namely, that such an arrangement would effect the most thorough and rapid

distribution of leaves around the stem. The latter part of this valuable paper consists of an attempt to show that the modes of phyllotaxy which result from the use of the different forms of the fraction are either directly serviceable to the plant by affording the best distribution, either for absorbing the sap from the roots or for exposing it to the action of air and light, or have been so at some period of the ancestry of the plant, when its structure was of a simpler character. The typical or unique angle of the theory of phyllotaxy the author regards to be the goal towards which the special forms tend, by the action of the principle of natural selection, rather than as the origin of the spiral arrangements.

A. W. B.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

The Law of Total Radiation

IN NATURE for April 25 Captain Ericsson attacks the calculation of Pouillet as to the sun's temperature, as being founded on an erroneous law of radiation. Had he contented himself with saying that the extension of Dulong and Petit's law so far beyond its experimental foundation to temperatures approaching that of the sun was "mere theory," and inconsistent with his own experiments, his position might have been impregnable. But not satisfied with this, he goes on to question the applicability of Dulong's law even below the boiling point of mercury, and asserts that Newton's law is much nearer the truth. The only objection that he gives to the method of the French experimenters is that they erroneously confuse the surface temperature of their thermometers with the average temperature of the contained mercury. The observed radiation is really due to the first, though attributed to the second. Now, without asserting that the objection is entirely without force, I submit that, if Newton's be the real law of radiation, it is impossible in this way to account for the observations.

In the first place, if the rate of cooling for a body at a variable temperature t , surrounded by another at a fixed temperature t_0 , be proportional to $t - t_0$, it follows from the theory of exchange (than which there are few things better established) that the radiation between two bodies at any temperatures t, t' , is proportional to $t - t'$. The rate of cooling of the thermometer contained in an enclosure would thus depend only on the excess of temperature, in flat contradiction to Dulong's observations. Nor would this result be altered, even though the material of the thermometer were so badly conducting in relation to its size as to allow the surface temperature to fall considerably below that of the interior. Whatever may be the relative temperatures after a given time of a system composed of a conducting mass, originally at a uniform temperature of 100° , surrounded by an enclosure maintained at 0° , the same after the same time will be the differential temperatures of the corresponding parts of another similar system, whose interior mass had originally a uniform temperature of 200° , with a case maintained at 100° . In fact, according to Newton's law, and with a constant conductivity, the superposition of any constant temperature over the whole system alters none of the conditions.

If it be objected that in the interior of a thermometer heat is distributed, not merely by conduction, but is convected by currents in the mercury, even this, I believe, will make no difference. The convection currents are a consequence of differences of density, and these are approximately proportional to the differences of temperature. The addition of a constant temperature to the whole alters nothing.

Judging from the evidence at present before us, it is impossible to avoid the conclusion that within the limits of Dulong's experiments Newton's law of cooling cannot be even approximately true. If Capt. Ericsson, by bringing forward fresh experiments, and by proving the fallacy of old ones, can establish the truth of Newton's law, he will lay Science under a great obligation. Speaking as a mathematician, I could even wish him success.

With regard to high temperatures it seems certain that Dulong and Petit's law fails; for it is impossible to believe that the sun

is no hotter than $1,500^\circ$ Cent., at least if the estimates hitherto made of terrestrial temperatures are substantially correct. It must be remembered, however, that according to Fizeau the sun is only about $2\frac{1}{2}$ times brighter than the electric arc, which does not even imply a higher temperature; because, while the sun must give us nearly the whole radiation due to his temperature, the electric arc is probably transparent.

J. W. STRUTT

Terling Place, Witham, April 29

Solar Halo

THIS morning, at 9.20, I observed a strongly-marked halo round the sun. Roughly extemporising a sextant with a post-card and paper-vector, I took three observations on the semi-diameter, and found the mean to be $22^\circ.6$. So I conclude this to be the ice-halo, whose deviation is 23° , being formed of hexagonal crystals. Two facts render the halo noteworthy—(1) the morning (after a heavy gale from the south) was exceptionally warm; (2) the halo exhibited the extreme colours in the proper order. I am told halos do not exhibit colours. Surely they ought to; and if not, why not? Let some of your readers answer me this. The halo was visible till nearly 10 o'clock.

Bournemouth, April 26

C. M. INGLEBY

Help us to save our Birds

ALL praise be given to those who have made a stand for the preservation of British birds. With a spirit of patience they have had to encounter the crass prejudice that sometimes saturates even the rural mind, and to prove that if the small bird takes its toll from, it also greatly assists in preserving the store of, the farmer. They have had to combat the sporting instincts of the excited townsman, so joyous with his escape from the smoky labyrinths of his brick-built prison that even a feathered shuttlecock would almost seem like game. Last and greatest feat, they have had to question the right to worship the national idol—gain, and to teach people, that even if, by the wholesale slaughter of feathered tribes, some persons scraped up gold, still that occupation, however praiseworthy, was against the general good. Truth at last dawned on the mind of the people, and so Parliament shielded, amongst other fowl, the pretty kittiwake from destruction, and preserved fashionable women from one more barbarism.

Those who have thus worked to educate the public need not rest on their oars for lack of employment, let them look farther afield, let them fearlessly step across national boundaries, and lend their strength to assist in arresting the impending destruction of many species of the most beautiful and interesting orders of animated nature in any quarter of the globe.

New Zealand, so long left by science to slumber on the calm bosom of the Pacific, has disclosed, amongst her birds, forms that have surprised the naturalist as much as they have excited the speculation of the philosopher. The remains of birds, of orders other than the gigantic Struthionies, giving us hints about strange lost forms of animal life that have lingered in these islands, perchance, almost to our own times, are now and then exhumed from the hidden shores of swamp and morass. We raise a cry for help in behalf of the mass of birds that yet remain near us (we had almost said *with us*), in the hope that the attention of naturalists in Europe may be called to the peril of extermination that hangs over many interesting indigenous species. For the preservation of our birds we require some assistance from abroad, our time is so crowded with occupations of many kinds, that without some pressure from without, little attention would be likely to be paid to the subject. This is said not without reason, not without some experience; in 1868, in Parliament, the writer tried to secure the conservation of our magnificent forests, a resolution was passed by the House to that effect, official inquiries were made;—*cui bono*? Our forests are now being damaged and destroyed, where not protected by climate, in so ruthless a manner, that no further evidence is needed to prove our wasteful style of settlement. Will not some one having authority in such matters speak a word in due season for our birds? I believe nearly every living species that we number could be preserved with proper care. If that is a fact, is it not interesting enough to naturalists to induce them to stimulate us to efforts more likely to give better results than our present legal enactments?